Private Enterprise Development in Low-Income Countries

Enterprise Responses to Redistribution in Kenya

Dennis Egger, Johannes Haushofer, Edward Miguel, Paul Niehaus and Michael Walker

We study a randomized, large-scale unconditional cash transfer program in Kenya, and find a meaningful increase in revenues for enterprises in areas experiencing a greater volume of cash transfers. Interestingly, sales increased without noticeable changes in firm investment behavior (beyond a modest increase in inventories), and sales do not increase differentially for firms owned by cash recipient households relative to non-recipients. Both patterns suggest a demand-led rather than an investment-led expansion in economic activity. Combining enterprise and household data, we estimate a cash transfer multiplier effect of 2.7.

Background

How redistribution affects the real economy is one of the central, unanswered questions in development economics. While a large literature has studied the impacts of transfers on recipient households, there is limited evidence on the interaction between redistribution and small- and medium-sized enterprises (SMEs) in low-income countries. More broadly, the supply-side responses to redistribution and social protection programs are not well-understood.

We combine data collected from households and enterprises (including both a census and surveys) with a large-scale randomized controlled trial to estimate the causal effects of redistribution in cash on enterprises. The nonprofit organization GiveDirectly (GD) delivers large unconditional cash transfers to poor households in rural Kenya. Villages are randomly assigned to treatment or control status. Within treatment villages, all eligible households – households with grass-thatched roof homes, used as a measure of poverty – are enrolled in GD's programme. These transfers total about USD 1,000 per household (nominal, i.e. not adjusted for inflation), and are distributed in three payments over 8 months. This accounts for around nine months of consumption for recipient households. About USD 11 million, over 15 percent of GDP in treatment villages, was distributed across 653 treatment and control villages.

Enterprises are not directly targeted, analogous to many social protection programmes in developing countries, yet may be affected by the large infusion of cash into the areas in which they operate. This project was designed to study the general equilibrium effects of cash transfers on enterprises, households, prices and local public finance.

Data and Methodology

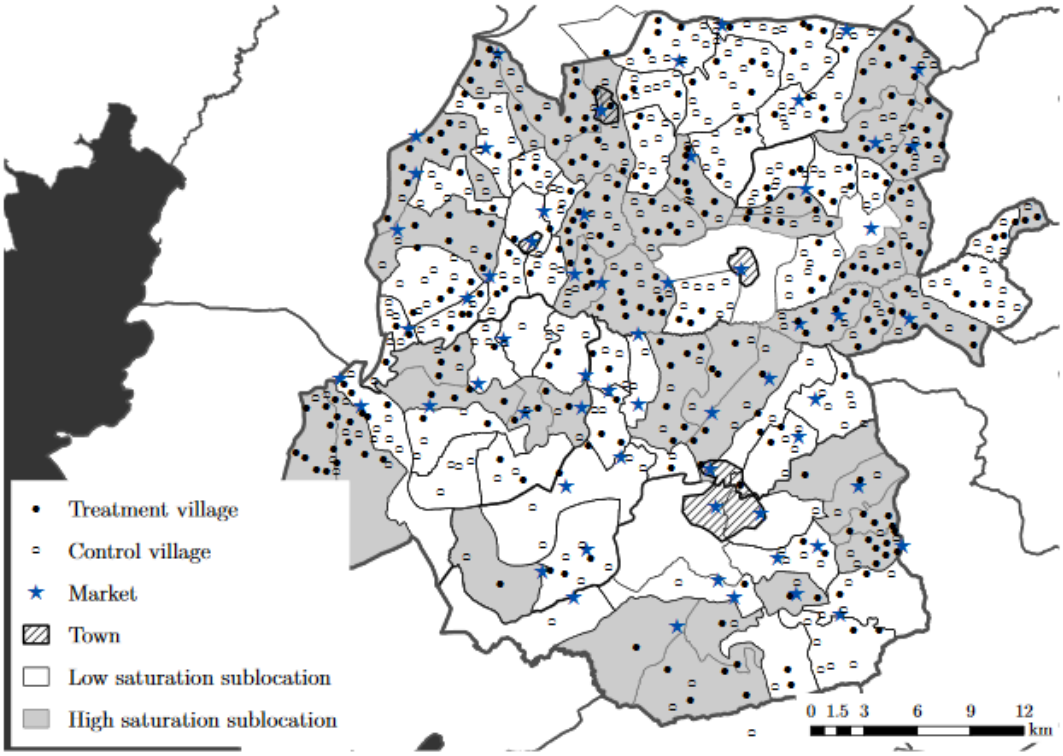
We conducted a census of enterprises after the distribution of the transfers, and randomly selected up to five enterprises per village (three operating from outside the homestead, and two operating from within homesteads) to be surveyed in more detail. (This builds on pre-treatment enterprise census and survey.) We can combine this with information from household surveys on agricultural activities to get a picture of all sectors of the economy. These activities took place between June 2016 and May 2017, roughly 1.5 years after the start of transfers. Our endline census captured over 12,000 non-farm enterprises, and our surveyed sample includes 1,699 enterprises operated from within and 1,442 from outside the homestead (both from enterprise surveys), as well as 7,899 agricultural enterprises from the household survey. These enterprises are generally small and informal, with the median enterprise having a single employee (i.e. the proprietor).

We measure (annualized) revenues and profits for non-agricultural enterprises directly by asking respondents about these. We calculate costs as the sum of the employee wage bill, rent and security costs;

this is not a comprehensive measure of all costs, and hence we do not expect the revenue measure to equal our measure of profits plus measured costs. For agricultural enterprises, total revenue is calculated as the sum of crop output (measured at the crop level) plus the value of pastoral and poultry output sold, and the value of the household's own consumption of pastoral and poultry output. Agricultural costs are the wage bill, all agricultural inputs (e.g., seed and fertilizer), and land rental costs. We then calculate agricultural profits as total agricultural revenue minus agricultural costs.

The randomized controlled trial design allows us to make several comparisons to study effects. First, we are able to compare enterprises in treatment villages to enterprises in control villages (column 1 of Table 1). In the absence of cross-village spillovers, this analysis would capture the causal effect of interest. However, as households shop across multiple villages, our preferred estimation strategy thus uses the spatial variation in treatment intensity generated by the RCT to estimate causal effects inclusive of spillovers. Some enterprises are located in areas that, by chance, are surrounded by many treatment villages, and others that are surrounded by few treatment villages (see Figure 1). We calculate these "total" effects separately for treatment (column 2) and control (column 3) villages.

Figure 1: Study area and design



Notes: This figure plots study villages, sublocation boundaries, and weekly markets in the study area in Siaya County, Kenya. Control villages are denoted by hollow circles, treatment villages are denoted by solid circles, and blue stars indicate the locations of markets. High saturation sublocations are shaded in gray, while low saturation sublocations are those in white. Town boundaries are shaded with diagonal lines.

Main Results

Our paper finds that recipient households report increased expenditure and asset ownership 1.5 years after the start of transfers, and that much of this money was spent locally. We also find no evidence of large price increases. What do these results mean for local enterprises?

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There are large increases in revenue for enterprises in both treatment and control villages (Table 1, Panel A). Revenues in treated villages increased by USD PPP 348 per household, a 46% increase, while those in control villages increased by USD PPP 231 (30%). Revenue gains are concentrated in the retail and manufacturing sectors. Estimated effects on profits are positive, but moderate in magnitude and not significantly different from zero. In fact, profit margins (measured as the ratio of profit to revenues) fell (Panel A, Row 5). We also see no evidence of firm entry, as one might have expected if enterprises were becoming more profitable (Panel C). Overall, the data indicate that higher revenues were largely absorbed by increased payments to various factors of production. While we do not directly observe all of these payments in our data, we do see significant increases in the factors that we directly measure, and particularly in the wage bill: enterprises in treated (control) villages increase spending on labor by USD PPP 85 (73), a sizable change relative to the control mean.

Table 1: Enterprise Outcomes

	(1)	(2)	(3)	(4)
	Treatment Villages		Control Villages	
	1(Treat village) Reduced form	Total Effect IV	Total Effect IV	Control, low saturation weighted mean (SD)
Panel A: All enterprises				
Enterprise profits, annualized	15.76	68.13	31.21	322.91
	(23.51)	(44.48)	(41.18)	(691.28)
Enterprise revenue, annualized	5.93	348.10**	230.98**	757.56
	(94.49)	(143.24)	(106.88)	(2,499.92)
Enterprise costs, annualized	−9.47	98.42**	79.96*	147.39
	(29.19)	(42.21)	(44.68)	(550.78)
Enterprise wagebill, annualized	−12.69	85.16**	73.08*	120.24
	(26.19)	(37.51)	(41.29)	(492.65)
Enterprise profit margin	0.01	−0.05**	−0.06***	0.44
	(0.02)	(0.03)	(0.02)	(0.61)
Panel B: Non-agricultural enterprises				
Enterprise inventory	10.85	34.69**	17.06	193.87
	(9.12)	(14.46)	(10.48)	(505.65)
Enterprise investment, annualized	4.05	13.38	6.69	179.49
	(7.08)	(15.54)	(8.70)	(644.15)
Panel C: Village-level				
Number of enterprises	0.01	0.01	0.00	1.12
	(0.01)	(0.02)	(0.01)	(0.14)

Notes: Column 1 reports the coefficient on an indicator for treatment village, and includes an indicator for saturation status of the sublocation. Column 2 reports the total effect on enterprises in treatment villages (own-village effect plus across-village spillover) from the "optimal" IV spatial regression of each outcome on the amount transferred per capita to a enterprise's own village v (instrumented by village treatment status), and to villages other than v in each 2km radii band around the enterprise (instrumented by the share of eligible households assigned to treatment in villages other than v inside the buffer). Column 3 reports the total effect on enterprises in control villages (across-village spillover only). For each column, we stack 3 separate regressions for own-farm enterprises, non-agricultural enterprises operated within the household, and non-agricultural enterprises operated outside the household, due to our independent sampling across these enterprise categories. We have between 10,015 and 10,284 observations for all enterprises, and 2,414 to 2,423 for variables we collect for non-ag enterprises only. The number of radii bands included in columns 2 and 3 is chosen, as pre-specified, by minimizing the BIC. Column 4 reports the weighted mean and standard deviations of the outcome variables in low-saturation control villages (across all enterprise categories). Each regression is weighted by inverse sampling weights and contains village-level baseline averages of the outcome variable by enterprise category when available. For monetary values, we convert effects to a per-household level by multiplying the average effect in each enterprise category by the number of enterprises in that category, dividing by the number of households in our study area, and summing over all enterprise categories. For the number of enterprises, we run regressions at the village level, where the outcome is the number of enterprises per household in each category, we weight by the number of households in each village and sum up over all enterprise categories. For the profit margin, we weight the effects across all enterprise categories by their share in the economy, and across each enterprise by revenue, so that our estimate represents the effect on the revenue-weighted average enterprise in the economy. Standard errors are clustered at the sublocation in column 1, and calculated following Conley (1999), 2008 using a uniform kernel out to 10 km in columns 2 and 3. * denotes significance at 10 pct., ** at 5 pct., and *** at 1 pct. level.

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Strikingly, we do not see strong evidence of a firm investment response. Estimated increases in fixed capital investment are small, and we can reject large changes (Panel B, Row 2). We do see a modest increase of USD PPP 35 in inventories for enterprises located in treated villages, yet even this appears to be less than proportional to the increase in firm sales; in other words, these enterprises are, if anything, operating leaner business models (Panel B, Row 1). This pattern of results suggests that the expansion in enterprise activity is driven more by the shock to local aggregate demand than by a relaxation of credit constraints that had previously limited investment.

A key question is how the economy absorbed such a large shock to aggregate demand, especially without changes in consumer prices. Real output increased, and yet there is at most limited evidence of increases in the employment of land (which is in fixed supply), labor, or capital. One plausible, albeit speculative, possibility is that the utilization of these factors was “slack” in at least some enterprises. This seems plausible because in the retail and manufacturing sectors, where output responses were concentrated, the typical firm has a single employee (i.e., the proprietor), and many enterprises operate “on demand” in the sense that they produce only when they have customers, and the average non-agricultural enterprise sees just 1.7 customers per hour. The existence of slack may help account for the large multiplier we document, as has also recently been argued in US data, especially in poorer US regions (Michaillat and Saez 2015, Murphy 2017, Chodorow-Reich 2019).

Implications for Policy

Finding a large multiplier effect from a cash transfer program, along with minimal price inflation, is of great policy interest for social protection programmes, and the extent to which these results can generalize to other settings is of particular interest. An especially noteworthy feature of our study setting is the fact that we estimate a large multiplier even during a period when the Kenyan economy was experiencing steady economic growth, rather than a recession. Hence it appears that any under-utilization of supply side capacity is not simply temporary or cyclical in rural Kenya, but rather may be more persistent, and an important feature to understand when setting private-sector policy. The fact that we do not find a large enterprise investment response is also notable for policymakers.

Additionally, an important feature of the enterprise results is that they are largely driven by the overall intensity of treatment in nearby communities, as opposed to the treatment status of the village in which the enterprise is located alone. This suggests that taking into account cross-village spillovers may be important for understanding the effects of programs on enterprises, at least in densely populated areas such as the one we study.

Moving Forward…

The results described here measure effects roughly 1.5 years after the start of transfers. An open question is whether these effects persist years after the transfers have ended. A traditional perspective would be that, once a local aggregate demand shock (like the cash transfers in our study) ends, the economy would eventually return to the previous steady-state. However, other theoretical perspectives from international trade, economic geography, and development, would suggest there could be persistent local effects of a temporary cash infusion, due to agglomeration effects, liquidity traps, increasing returns, changes in income inequality, market structure and firm specialization, and even shifts in the social networks of traders and suppliers. With co-funding from PEDL, we are investigating the longer-term effects of redistribution on enterprises 4-5 years after the start of transfers via another round of enterprise censusing and surveying. We plan to focus more attention on measuring capacity utilization to better understand how this operates in a rural economy as well.

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